EXHIBIT 114

UNITED STATES DISTRICT COURT
NORTHERN DISTRICT OF ILLINOIS
EASTERN DIVISION

ANDREW CORZO, SIA HENRY, MICHAEL MAERLANDER, ALEXANDER LEO-GUERRA, BRANDON PIYEVSKY, BENJAMIN SHUMATE, BRITTANY TATIANA WEAVER, and CAMERON WILLIAMS, individually and on behalf of all others similarly situated,
 	
    Plaintiffs,
 	
v.
 	
BROWN UNIVERSITY, CALIFORNIA INSTITUTE OF TECHNOLOGY, UNIVERSITY OF CHICAGO, THE TRUSTEES OF COLUMBIA UNIVERSITY IN THE CITY OF NEW YORK, CORNELL UNIVERSITY, TRUSTEES OF DARTMOUTH COLLEGE, DUKE UNIVERSITY, EMORY UNIVERSITY, GEORGETOWN UNIVERSITY, MASSACHUSETTS INSTITUTE OF TECHNOLOGY, NORTHWESTERN UNIVERSITY, UNIVERSITY OF NOTRE DAME DU LAC, THE TRUSTEES OF THE UNIVERSITY OF PENNSYLVANIA, WILLIAM MARSH RICE UNIVERSITY, VANDERBILT UNIVERSITY, and YALE UNIVERSITY,
 	
    Defendants.
	Case No. 1:22-cv-00125
	
	Class Action
	
	SUPPLEMENTAL REBUTTAL EXPERT REPORT OF HAL J. SINGER, PH.D.
	
	November 8, 2024

This report cites and quotes material that Defendants designated as
Confidential or AEO under the Second Amended Confidentiality Order (ECF No. 608)

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INTRODUCTION AND SUMMARY OF CONCLUSIONS

1.   On October 7, 2024, I submitted a Rebuttal Expert Report ("Rebuttal Report" or "Singer Rebuttal Report") in response to Defendants' Experts' reports.[1] Defendants' Experts submitted sur-rebuttal reports on November 1, 2024. Counsel for Plaintiffs have asked me to review the expert sur-rebuttal reports of Dr. Nicholas Hill[2] and Dr. Lauren Stiroh[3] and to respond to their opinions. I find that none of Defendants' Experts' sur-rebuttal arguments has any merit, nor does any argument alter my opinions or conclusions.

2.   In my Initial Report,[4] among many other analyses, I used a standard two-step approach to demonstrate common impact. The multiple analyses that were part of each step of my common impact proof are common to the Class as a whole.[5] The first step involved, among other analyses and evidence, applying generally accepted statistical techniques (namely, regression modelling) to determine whether the Challenged Conduct resulted in artificial inflation of Defendants' Effective Institutional Prices.[6] The second step involved, among other evidence and analyses, using my regression model to demonstrate classwide impact via in-sample prediction, price-structure regressions, and a common shock analysis.[7] In my Rebuttal Report, in response to specific criticisms of Defendants' experts, I applied similar regression modeling techniques to determine whether the Challenged Conduct artificially inflated Expected Family Contributions (EFCs).[8] I found that it did. I also applied regression modeling techniques to determine whether the Challenged Conduct resulted in a reduction in the range of EFCs and Effective Institutional Prices

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1.   Rebuttal Expert Report of Hal J. Singer, Ph.D. (Oct. 7, 2024). I use the terminology "Rebuttal Report" when referring to this report within the body text, and otherwise refer to it as "Singer Rebuttal Report" in footnote citations.
2.   Surrebuttal Expert Report of Nicholas Hill, Ph.D. (Nov. 1, 2024) [hereafter Hill Surrebuttal Report].
3.   Surrebuttal Expert Report of Lauren J. Stiroh, Ph.D. (Nov. 1, 2024) [hereafter Stiroh Surrebuttal Report].
4.   I use the same defined terms here as I had used in both my Initial and Rebuttal Reports.
5.   Singer Report ¶13.
6.   *Id*. §III.A.
7.   *Id*. §III.B.
8.   Singer Rebuttal Report ¶44 and Table 1.

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for students that were cross admitted to multiple Defendants.[9] I found that it did. These findings are consistent with Plaintiffs' allegations of conspiracy and inconsistent with competition.[10]

3.   Defendants' Experts have presented arguments in their sur-rebuttals intended to undermine my findings. Their arguments are either inconsequential to my overall opinions, or are statistically or otherwise misguided, or both.

4.   Dr. Hill's critiques have been exclusively focused on the regression modelling that is part of the first step of my standard two-step approach to proving common impact; he does not assess my findings regarding the extent of impact experienced across the Class. His arguments are generally focused on either: (1) data processing; (2) standard errors (i.e., "statistical inference"); or (3) econometric implementation. With regard to (1), I have already accepted a number of data processing adjustments that Dr. Hill had proffered in his August 7, 2024 Expert Report ("Dr. Hill's Initial Report").[11] He takes issue with the fact that I do not accept two of the over 60 data processing changes that he provided in his Initial Report. I provide justification both in my Rebuttal Report and below in part I.A explaining why these two proposed changes are improper.

5.   Dr. Hill's standard error arguments, which he uses in an effort to deflate the statistical significance of my findings, are inapposite. Even if I accepted his flawed standard errors approach (which I do not), my overcharge model still continues to show a positive, economically, and statistically significant conduct coefficient.[12] In other words, his standard error critique does not vitiate my central finding that the Challenged Conduct had an economically and statistically significant impact on both Effective Institutional Prices and EFCs. In any event, I disagree that his two-way clustered standard errors are proper in the instant case. The standard errors wrongly assume

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9. *Id.* ¶¶45-46 and Tables 2-3.
10. *Id.* ¶¶44-46 and Tables 1-3.
11. *Id.* §III.A.2.c.
12. *See* Part I.B., *infra*.

that the Challenged Conduct consisted of a series of sequential but wholly independent conspiracies hatched and re-hatched by Defendants in each academic year. And that assumption has no basis in fact as I understand the record, or in Plaintiffs' allegations regarding the Challenged Conduct, which consists of a single Overarching Agreement spanning nearly two decades. The fact that some Defendants joined or claimed to have abandoned the Challenged Conduct at different dates does not change the allegation itself that there is a single body of Challenged Conduct, which is the allegation that my regressions tested, not the alternative hypothetical multiple conspiracies posited by Dr. Hill.

6.   For ease of exposition, I refer to other critiques brought forth by Dr. Hill as issues related to "econometric implementation."[13] I responded to these in my Rebuttal Report, and so I will not discuss them here, except for the two that Dr. Hill opines on in his sur-rebuttal. In his sur-rebuttal, Dr. Hill relitigates what he refers to as "sensitivity analyses." But these critiques amount to improper "data dredging" because they include invalid control variables and remove relevant benchmark data.[14] Data dredging refers to the improper use of statistical techniques designed to generate the results a researcher wants to find, which, as I discuss in more detail below, is what Dr. Hill has done in his reports here. Dr. Hill also argues that some of the alternative regressions that I had provided in my Initial Report produce inconsistent results when they are applied to my amended data or to my EFC regression analyses.[15] Dr. Hill is wrong. He both misconstrues those of my models that are actually relevant for my damages analysis, and he is wrong about the supposed inconsistency.[16] In short, I find Dr. Hill's "econometric implementation" arguments to be meritless.

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13. I will use this term to refer to any issue that Dr. Hill raises unrelated to data processing or standard errors. For instance, Dr. Hill introduces alternative control variables into my regressions, asserts that I should have removed what he deems to be "flawed" post-2022 data, claims that my use of student-fixed effects leads to flawed results, asserts that my regression model produces counterintuitive results due to staggered treatment effects, and claims that my regressions were subject to selection bias. *See* Hill Report §§8.3-8.5.

14. *See* Part I.D; Singer Rebuttal Report §§III.A.2.d-e.

15. Hill Surrebuttal Report §4.

16. *See* Part I.C, *infra*.

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7.   Dr. Stiroh's arguments are primarily focused on the second step of my two-step approach—showing that the impact of the Challenged Conduct was experienced broadly across the Class. In both Dr. Stiroh's August 7, 2024 Expert Report ("Dr. Stiroh's Initial Report") and her sur-rebuttal report, she attacks my standard in-sample prediction methodology on the ground that it purportedly imposes a common impact rather than tests for it.[17] Dr. Stiroh's additional arguments in the sur-rebuttal regarding my "harmed on net methodology," are equally flawed as her initial arguments.[18] Both her new and prior arguments fundamentally misuse the in-sample test. As discussed in my Initial Report, in-sample prediction enjoys widespread use: "[c]ourts have certified several antitrust class action cases where I, as well as other Plaintiffs' economists, have reliably used this method [in-sample prediction] to demonstrate common impact."[19] Her attacks are predicated on proceeding to the second part of the in-sample analysis even if the first step shows a generalized overcharge of zero. That is erroneous. If it were the case that the generalized overcharge from my impact regression were zero, the analysis would stop there. Nevertheless, in Part II.C, I present an alternative version of my in-sample prediction that addresses Dr. Stiroh's refashioned in-sample prediction critiques, and I find that her critiques do not alter my finding that the in-sample technique, combined with my other analyses of classwide impact, demonstrate that all or nearly all Class Members were impacted by the Challenged Conduct.

8.   In Dr. Stiroh's sur-rebuttal report, she also critiques my EFC regressions for not establishing classwide impact.[20] Dr. Stiroh's critiques have no effect on the analyses and are thus

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17. Stiroh Surrebuttal Report ¶16; Stiroh Report §VII.C.1.
18. *See* Singer Rebuttal Report ¶¶225-238 for an explanation of the flaws in Dr. Stiroh's critiques of my in-sample prediction methodology.
19. Singer Report ¶256 and n. 353 (citing to *Johnson v. Arizona Hospital & Healthcare Ass'n*, No. CV 07-1292-PHX-SRB, 2009 WL 5031334 (D. Ariz. Jul. 14, 2009); *In re High-Tech Employees Antitrust Litigation*, 985 F. Supp. 2d 1167 (N.D. Cal. 2013)).
20. See Singer Rebuttal Report ¶¶44-46 and Tables 1-3; Stiroh Surrebuttal Report §III.A.

inconsequential. I have already established that all or nearly all Class Members had been impacted via higher Effective Institutional Prices. EFCs are but one component of Effective Institutional Prices, which are ultimately what matter for showing antitrust effects. Dr. Stiroh also erroneously asserts that I have not established that packaging was affected by the Challenged Conduct, when the record evidence and my economic analyses demonstrate that the affordability principle (and other aspects of the Challenged Conduct) would likely have affected packaging.[21]

I.   DEFENDANTS' EXPERTS' ADDITIONAL CRITIQUES OF MY IMPACT REGRESSIONS ARE UNAVAILING

9.   With regard to the first step of my two-step approach to demonstrating common impact, Defendants' Experts largely repeat critiques from their initial reports regarding my Effective Institutional Price regressions, and thus do not respond to supposed new arguments advanced in my Rebuttal Report. Defendants' Experts also recycle the same flawed arguments but this time direct them towards my EFC regressions. I respond to each of their recycled arguments below.

A.   Dr. Hill's Amended Data Are Flawed and Result in Unreliable Overcharge Estimates

10.   In Dr. Hill's Initial Report, he made several changes to how I processed Defendants' structured data into the dataset used for my impact regressions and damages calculation.[22] In my Rebuttal Report, I accepted the overwhelming majority of these changes, and I found that they generally had a trivial to modest effect on my overcharge results.[23] I noted that there were two changes that Dr. Hill had made that I disagreed with, and I showed that his implementation of these two improper changes significantly decreased his model's overcharge estimates. In Appendix C of Dr. Hill's sur-rebuttal report, he asserts that my dismissal of these two changes was improper, with the exception of a minor adjustment to which award payment field he used in Chicago's 2022 data.

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21. Singer Report ¶¶188-191.
22. Hill Report ¶109.
23. Singer Rebuttal Report §III.A.2.c.

I disagree with Dr. Hill. I find that his arguments do not nullify the evidence put forward in my Rebuttal Report for (1) excluding Chicago's flawed post-2015 data, and (2) using Duke's "accepted" amount field for measuring Duke's financial aid awards.[24]

1. Dr. Hill's Justifications for Including Chicago's Post-2015 Flawed Data Are Invalid

11.   In my Rebuttal Report, I showed that there were notable problems with Chicago's post-2015 structured data that could be attributed to a switch in their financial aid database systems. Specifically, I showed that between 2015 and 2016, there was a prominent spike in Chicago's average Effective Institutional Price from ▆▆▆▆ to ▆▆▆▆ and another spike between 2021 and 2022 from ▆▆▆▆ to ▆▆▆▆.[25] This spike does not reflect an actual change in Effective Institutional Prices, rather, it is the result of the change in database systems between 2015 and 2016. I explained that including this invalid data causes a significant downward bias in my conduct coefficient estimates. I thus excluded Chicago's post-2015 data for this reason.

12.   Dr. Hill claims that my decision to exclude Chicago's post-2015 data is flawed.[26] But he never actually addresses the key argument that I raised—namely, that there is a material increase in Chicago's Effective Institutional Prices that coincides exactly with the 2015 to 2016 database change and does not correspond to reality.[27] He never explains why this substantial increase in Effective Institutional Prices would reflect an actual increase in Effective Institutional Prices, rather

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24. In Appendix C.3 of Dr. Hill's Surrebuttal Report, he states that I had not corrected an error in how he classified Emory BBA and Education Abroad awards. This adjustment makes no material difference—these BBA and Education Abroad awards comprise 226 out of over 277,000 awards observations.
25. Singer Rebuttal Report ¶111.
26. Hill Surrebuttal Report ¶¶108-110.
27. Based on publicly available sources, this sudden increase in Effective Institutional Price does not reflect reality. For example, Data USA (using IPEDS data) found that the average net price for University of Chicago students was approximately $33,107 in 2015, while in 2016 the average net price was $31,068, showing that the average net price actually *decreased* between 2015 and 2016. Note that these are nominal values; using real 2024 dollars would result in higher values. Also note that only net price is reported, which slightly differs from Effective Institutional Price (net price also accounts for non-institutional grants and loans), but is likely to be highly correlated with Effective Institutional Price. *See University of Chicago Average Net Price*, DATA USA, https://datausa.io/profile/university/university-of-chicago (last visited Nov. 8, 2024).

than a statistical irregularity resulting from the database change. Nor does he address my arguments that a significant discrepancy such as this would bias the conduct coefficient of my primary impact regression downwards unjustifiably. Instead, he pivots to only accept my claim that there is a problematic jump in Effective Institutional Prices in 2022, and he only adjusts his processing of Chicago's 2022 awards data to smooth out this jump.[28]

13.   Dr. Hill claims that my decision to omit Chicago's post-2015 data is inconsistent with my treatment of data from other Defendants that had switched database systems.[29] His argument is flawed because it ignores the underlying issue with the database switch in this particular instance. The key difference between Chicago's database switch and other Defendants' database switches is that other Defendant switches did not result in a significant discrepancy in how Effective Institutional Prices were measured over time, whereas Chicago's database change did. This argument also ignores that Chicago's discrepancy in Effective Institutional Prices coincided directly with when Chicago left the cartel. Given the high correlation between when Effective Institutional Prices became mismeasured and when Plaintiffs credited (for purposes of the Class definition) Chicago's leaving of the 568 Group, any issue with mismeasurement leads to significant bias in the conduct coefficient.

14.   Dr. Hill also makes the inapposite claim that my removal of Chicago's post-2015 data is incompatible with my claim that one should not ignore limited data if the researcher is unable to obtain complete data.[30] Yet this is not an issue of incomplete data—the Chicago post-2015 data is complete. Rather, the problem is that the data are flawed and thus do not allow one to measure the variable of interest consistently over time. And, unlike in other scenarios where one may have

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28. Hill Surrebuttal Report ¶110.
29. *Id.* ¶109.
30. *Id.* ¶109.

limited data yet have no basis to conclude that said data is flawed, here I am aware of and can ascertain the mismeasurement issue, and I am aware of the direction of bias that it creates. Specifically, I observe that the increase in Chicago's Effective Institutional Prices is correlated with my conduct variable, and that this increase is due to the switch in database systems. I was therefore correct to remove this data.

15.   In my Rebuttal Report, I stated that Dr. Hill dropped 38 percent of observations in the post-2015 data that had a missing value in the variable that he uses to identify the source of the award.[31] Dr. Hill clarifies that he did not fully "drop" these data, but instead classified these awards as being from an "other" source.[32] Dr. Hill's assumption that these awards merely come from "other" sources besides Chicago is unfounded. Effective Institutional Price is calculated by subtracting institutional grant aid from cost of attendance. It is therefore of vital importance that I can identify *institutional* awards versus *non-institutional* awards. That the variable in question was missing its source for 38 percent of observations means that the actual institutional grant aid awarded by Chicago could have been substantially higher after Chicago had purportedly left the 568 Group than what Dr. Hill calculates. This is likely, at least in part, why the mismeasurement of Effective Institutional Price between the pre-2015 and 2016-2022 data occurs. However, I cannot know by how much Chicago's Effective Institutional Price was post-2015 because the data do not specify.

2.   Dr. Hill's Justification for Using Duke's "Paid" Amount Instead of "Accepted" Amount Is Invalid

16.   In Dr. Hill's Initial Report, he had altered my processing of Duke's structured data to use a variable for "paid" amount rather than "accepted" amount. In my Rebuttal Report, I showed that using the "paid" amount was improper because it did not align with what was actually awarded

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31. Singer Rebuttal Report at n. 248.
32. Hill Surrebuttal Report ¶109.

to named plaintiff Sia Henry based on her interrogatory responses, whereas the "accepted" amount matched exactly to what she was awarded. Dr. Hill asserts that I am wrong because Sia Henry's responses were in relation to how much she was *offered* by Duke, but not necessarily by how much she was *awarded*.[33] It bears noting that Dr. Hill's statement conflicts with Dr. Stiroh, who stated in her Initial Report: "Ms. Henry was *awarded* need-based aid from Duke totaling $9,300 for the 2007–2008 academic year, $14,200 for the 2008–2009 academic year, $40,455 for the 2009–2010 academic year, and $30,579 for the 2010–2011 academic year."[34]

17.   The data are inconsistent with Dr. Hill's assertion that Duke's "paid" amount reflects the actual awarded amounts received by Class Members. For instance, there would be no rationale to expect that most or even that a small fraction of students that attended Duke would choose not to receive gift aid that was awarded to them by Duke, as gift aid is effectively "free" money, in contrast to loans or work study, which some students might rationally decline. When I compare the total institutional *gift* aid recorded for Class Members using Duke's "accept" amount variable (which I used) with the "paid" amount variable (which Dr. Hill uses), I find that the "accept" institutional gift aid amount shows up as greater than or equal to the "paid" institutional gift aid amount for 93 percent of awards observations.[35] Further, I find that 19 percent of observations with a positive "accept" institutional gift aid amount show up as having zero "paid" institutional gift aid. Both of these findings are consistent with the "accept" amount reflecting the actual award amount, otherwise Class Members would be leaving money on the table.

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33. *Id.* ¶113.
34. Stiroh Report ¶18.
35. *See* my workpapers. I calculate total institutional gift aid as the sum of institutional need-based and merit-based grants. I use both need-based and merit-based institutional gift aid for this analysis since both forms of aid are gift aid, and since one would therefore expect a student to accept these aid forms given that they do not need to be paid back.

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18.   Dr. Hill claims that I had not consistently used "accept" versus "paid" as a field for other Defendants.[36] His argument is irrelevant. For Defendants that had differences between "paid" and "accepted" amounts, the frequency of observations that differed across these two variables was generally substantially lower compared to Duke. Table 1 shows the percentage of processed data observations where Effective Institutional Price calculated using the "paid" amount differed from using the "accepted" amount by Defendant. Duke has the highest proportion of observations where Effective Institutional Price was affected by this payment variable discrepancy, with over 63 percent of Duke's regression observations showing a different "paid" Effective Institutional Price compared to using its "accepted" Effective Institutional Price variable. These results show that Dr. Hill's argument is irrelevant—the choice between "accepted" and "paid" amount makes little difference for most Defendants.

TABLE 1: PERCENT OF REGRESSION OBSERVATIONS THAT HAVE AN EFFECTIVE INSTITUTIONAL PRICE CALCULATED USING "ACCEPTED" AMOUNTS NOT EQUAL TO "PAID" AMOUNTS

Defendant	% of Observations Where "Paid" EIP Not Equal to "Accepted" EIP
Brown	0.0%
Caltech	0.0%
Chicago	3.4%
Columbia	1.3%
Cornell	20.7%
Dartmouth	0.1%
Duke	**63.1%**
Emory	0.0%
Georgetown	0.8%
JHU	0.0%
MIT	0.5%
Northwestern	0.1%
Notre Dame	0.0%
Penn	1.0%
Rice	0.0%
Vanderbilt	0.8%
Yale	0.0%

Notes: I calculate the share of regression observations that do not have a matching "paid" and "accept" amount by creating two versions of Dr. Hill's "Corrected regression data.dta," one version using "paid" amounts and the other using "accept" amounts. *See* my workpapers.

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36. Hill Surrebuttal Report ¶112.

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B.   Dr. Hill's Rehashed "Standard Error" Arguments Are Still Without Merit

19.   In Dr. Hill's sur-rebuttal, he reargues issues pertaining to standard errors that he had previously brought up in his Initial Report.[37] He claims that I had made new assertions about the subject.[38] I will again briefly explain my opinion for why these standard error critiques are inapposite.

20.   Importantly, I observe at the outset that Dr. Hill's standard error critiques do not change the conduct coefficient in my regression models, nor do they alter the statistical significance of my primary regression overcharge coefficient. The latter point is particularly relevant because the entire point of Dr. Hill's standard error critiques is to assert that my models lack statistical significance. Yet Dr. Hill is unable to get my primary overcharge model to switch from statistically significant to not-statistically significant and is therefore unable to score any points with this critique relevant to any issue in the case. In row one of his sur-rebuttal Figure 7, Dr. Hill finds that my primary overcharge model is both economically and statistically significant at a *one percent* significance level using his purported "corrected" two-way clustered standard errors. In short, Dr. Hill devotes such a significant volume of both of his reports towards an arcane academic fight about an issue that does not alter my findings at all. (In my entire litigation career, I cannot recall a Defendant's expert in any case, let alone a price-fixing case, devoting so much attention to such an arcane issue.).

21.   Dr. Hill misconstrues my Rebuttal Report response to his standard error criticisms. He claims that, in my Rebuttal Report, I offered a new idea regarding clustering. I addressed clustering in my Rebuttal Report only in response to Dr. Hill's opinion that I should have used clustered standard errors. I had no reason to address that issue in my Initial Report because I used

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37. *Id*. §3.
38. *Id*. §3.1 ("Dr. Singer's novel assertions about his standard errors are flawed[.]").

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Huber-White standard errors. Huber-White standard errors account for potential variance in the error term of the regression model, and as such should be used when the distribution of the error term is unknown.[39] I explained the use of Huber-White standard errors in my Rebuttal Report.[40] Dr. Hill appears to manufacture the theory that if I do not also discuss methods that I did not use in my Initial Report, responding to rebuttals that I should have used such methods constitutes a "novel" (or newly introduced) opinion. (Even if it were "novel," the purpose of his second report was to provide new empirical results, not new arguments.) I did not use clustering because (1) I did not find it to reflect an appropriate improvement over Huber-White standard errors in this case, and (2) more importantly, clustering in the manner that Dr. Hill proposes does not reflect the actual treatment assignment mechanism.

22.   Consideration of standard errors informs the precision of an estimated effect, the corresponding p-value, and thus the categorization of such an effect as "statistically significant" or not. The precision of the effect depends on the uncertainty in the estimation process. Such uncertainty originates from two key sources: (1) sampling-based uncertainty, and (2) design-based uncertainty.[41] Generally, researchers assume that the data used in their analyses represents a sample from some larger population, and thus the standard errors they report represent that sampling uncertainty.[42] This scenario does not apply here, as the data I have analyzed reflects a census of all Defendant data available. My analysis does not aim to extrapolate to any other Defendant or school

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39. JEFFREY WOOLDRIDGE, INTRODUCTORY ECONOMETRICS: A MODERN APPROACH (South-Western Cengage Learning 5th ed. 2013) [hereafter WOOLDRIDGE] at 269 ("The methods in this section are known as *heteroskedasticity-robust* procedures because they are valid—at least in large samples—whether or not the errors have constant variance, and we do not need to know which is the case."). *See also* WOOLDRIDGE at 272 (Robust (Huber-White) standard errors "are valid (asymptotically) whether or not heteroskedasticity [variance in the error term] is present.").

40. Singer Rebuttal Report ¶90.

41. Alberto Abadie, Susan Athey, Guido Imbens, and Jeffrey Wooldridge, *Sampling-Based Versus Design-Based Uncertainty in Regression Analysis*, 88(1) ECONOMETRICA 265-296 (2020).

42. *Id*. at 265.

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outside the scope of the data that I use. Dr. Hill acknowledges this reality in note 26 of his sur-rebuttal.

23.   Ultimately, the debate that Dr. Hill and I have over standard errors amounts to a dispute over the level of "treatment assignment" (i.e., the level at which individual Class Members were exposed to the Challenged Conduct). Dr. Hill's use of two-way clustered standard errors implies that the treatment should be viewed as having been assigned independently *per Defendant* and *per academic year*. I disagree with these presumptions. The central allegation of this case—a single overarching conspiracy spanning nearly two decades—implies that the treatment was not assigned independently per Defendant; nor do I find it to be the case that the treatment was assigned independently per academic year. Without a strong basis for these two assumptions, Dr. Hill's two-way clustered standard errors amount to artificially removing a large number of observations with the effect of artificially reducing the regression p-values.

24.   Dr. Hill suggests that one of the "two-ways" that treatment was assigned here is at the Defendant level, but the case at hand reflects a census of individual students at each of the Defendant schools, *all* of whom participated in the Challenged Conduct. The 568 Group was not a separate conspiracy per Defendant. It would not make sense to claim that a single Defendant acted unilaterally to conspire *with itself* to artificially inflate Effective Institutional Prices. As it takes two lovers to kiss, it takes two (or more) companies to form a cartel. Every Defendant allegedly participated in the same conspiracy at one point or another. Furthermore, the Challenged Conduct did not involve a random sample of Class Members each assigned either to a Defendant or a non-Defendant.

25.   Dr. Hill also suggests, besides treatment being assigned at the Defendant level, it should also be assigned at the academic year level. This assumption ignores that the alleged

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Overarching Agreement did not amount to a series of independent conspiracies stacked up year after year—rather, the single conspiracy spanned continuously nearly two decades. Dr. Hill's clustering by year also ignores the fact that the treatment assignment spanned multiple years per Class Member. For example, a student who matriculated to Chicago in 2015 (the last year of participation for that Defendant) would have suffered harm in 2015 but that harm could have extended to subsequent years through lock-in effects and spillover effects. Dr. Hill's argument also amounts to claiming that Defendants "randomly" chose whether to participate in the Challenged Conduct in each year, such as via a coin toss. While I agree that Defendants did participate in certain years and not in others, the pattern of participation was not random. Rather, most Defendants had maintained continuous participation in the 568 Group over prolonged time frames before leaving, and did not randomly enter-and-exit as a true random assignment by year would imply.

26.   To illustrate, consider the difference between the instant case and a minimum-wage policy implemented in a given state or a school district adopting an experimental educational plan. In the educational example, the treatment assignment could occur at the individual cluster level. In contrast, in the context of an alleged collusive agreement, a cartel of one entity cannot exist. By definition, a cartel such as that alleged of the 568 Group involves a collusive agreement among multiple parties. We could not assign membership in the 568 Group only for Brown or only for Duke or only for Northwestern. Moreover, if only a relative minority of the Defendant schools participated in the 568 Group, the cartel would likely dissipate in short order, as competition from other now-Defendants would police such conduct. Thus, the Challenged Conduct reflects a cartel-level treatment assignment, not a Defendant-year treatment assignment as Dr. Hill asserts. Dr. Hill's two-way clustering does not reflect the actual treatment assignment. Dr. Hill does not get to redefine the Challenged Conduct in this case. His clustering methodology only serves to artificially reduce the

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sample size, in an attempt to undermine the statistical significance of my findings. As such, I reject Dr. Hill's approach as inaccurate and inconsistent with the facts of the case.

C.   Dr. Hill's Assessment of My Alternative Regression Models Is Baseless

1.   Dr. Hill Misconstrues the Purpose of My Alternative Regression Models

27.   In both my Initial and Rebuttal Report, I consistently referred to the conduct coefficient from what I deem to be my primary regression model as my preferred measure of the artificial overcharge amount. I had only used this primary regression model specification for purposes of demonstrating in-sample prediction, running my price-structure regressions, and for computing aggregate damages. My primary regression incorporates all of the information and methods necessary to achieve the most robust identification of the Challenged Conduct's effect on Effective Institutional Prices. I also presented alternative regressions to illustrate how results are affected as more nuance is added into the analysis, with the most nuance being captured by my primary regression model. This included presenting six different specifications in my primary regression table (Table 11 of my Initial Report), plus including alternative versions of these six specifications in the Appendix of my Initial Report. Table 2 outlines the alternative Appendix models. Dr. Hill asserts that my damages calculations change materially in response to which alternative regression is used. But my primary model is what counts.

TABLE 2: ALTERNATIVE MODELS PRESENTED IN MY INITIAL REPORT

Initial Report Table	Difference with Primary Regression Table
Appendix 4 Table 1	Uses COA minus both need-based *and merit-based* institutional gift aid as dependent variable
Appendix 4 Table 2	Uses log-linear specification (as opposed to levels)
Appendix 4 Table 3	Uses COA minus both need-based *and merit-based* institutional gift aid as dependent variable; Uses log-linear specification (as opposed to levels)
Appendix 5 Table 1	Uses institutional grant aid as the dependent variable instead of Effective Institutional Price, and includes the tuition CPI as a control variable
Appendix 5 Table 2	Uses institutional grant aid as the dependent variable instead of Effective Institutional Price, and it includes the tuition CPI as a control variable. Institutional grant aid includes both need-based *and merit-based* institutional gift aid.

28.   In Part 4 of Dr. Hill's sur-rebuttal report, he computes damages using the alternative models listed above using my amended data.[43] He states that the "six specifications produce conflicting results both within and across Dr. Singer's five sensitivity tests."[44] He also claims that my primary model is not conservative because some of my sensitivity tests produce results that would result in lower damages.[45]

29.   Dr. Hill's arguments are misguided because they are based on the presumption that all of my alternative specifications need to be consistent with my primary regression model. I presented what I refer to as my "primary regression model" in my Initial Report for a specific reason—because it directly and most accurately measures the artificial overcharge in Effective Institutional Prices that resulted from the Challenged Conduct. To reflect best econometric practices, I presented a range of alternative specifications in each of my regression tables to demonstrate how results changed as more control variables were included, but this does not mean that these alternative models should consistently mirror the direction or magnitude of my primary model estimates across

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43. Hill Surrebuttal Report Figure 3.
44. *Id.* ¶36.
45. *Id.* ¶41.

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all specifications.[46] In fact, there is a good reason why the alternative specifications should not be consistent in their sign or magnitude as the primary model—specifications 1-5 of my regression tables do not control for all relevant factors that may confound the causal relationship between the Challenged Conduct and Effective Institutional Prices. Specifications 1-3 of my regression tables use Defendant fixed effects but do not include student-level fixed effects. This omission means that, practically speaking, specifications 1-3 have over 200,000 fewer control variables than specifications 1-6, and are therefore less likely to produce an unbiased measure of the effect of the Challenged Conduct on Effective Institutional Prices compared with specifications 4-6.[47] Further, I iteratively include additional control variables as one moves from specification 1 to 3 and from specification 4 to 6 in Table 11 of my Initial Report; I start with only student-level controls, then I add institutional-level controls, then I add macroeconomic controls. Specification 6 provides the most robust specification for blocking any back-door paths through which confounding factors may bias the conduct coefficient, hence why I use it for computing damages.

30. Dr. Hill ignores the justification that I provided in my Initial Report for using the conduct coefficient from my Effective Institutional Price model instead of from my institutional grant aid model:

> Ultimately, prospective students and their parents focus on the price they have to pay for an education, hence the Department of Education's requirement of a net price calculator. The conduct coefficient in regressions using Effective Institutional Price capture the effect of any element of the Challenged Conduct on the outcome of interest. The effect of the Challenged Conduct on institutional grant aid reflects only an *intermediary* impact. Students and parents compare their out-of-pocket costs across schools rather than the relative amount of institutional grant aid.[48]

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46. *See* Laura Alfaro, Sebnem Kalemli-Ozcan, and Vadym Volosovych, *Why doesn't Capital Flow from Rich to Poor Countries? An Empirical Investigation*, 90(2) THE REVIEW OF ECONOMICS AND STATISTICS 347-368 (2008) (Table 3, displaying results under different specifications. The results are of different magnitudes and directions under varying specifications, such as GDP per capita being inconsistent in sign or statistical significance).

47. My student-fixed effects models in Table 11 columns 4-6 from my Initial Report include a dummy variable for every student-Defendant combination, whereas columns 1-3 only contain dummy variables for each Defendant.

48. Singer Report ¶283 (emphasis added).

Dr. Hill instead asserts that results from my institutional grant aid regression should be equivalent to results from my Effective Institutional Price regression. Institutional grant aid functionally acts as a discount to price. My goal here was to show an effect on the net price paid, and not merely on the effect of the Challenged Conduct on discounts or certain price elements. Hence, I use the artificial overcharge in Effective Institutional Prices for the purpose of computing damages.

31.   It bears noting that there is a sound economic basis for explaining that the conduct coefficient of my primary Effective Institutional Price regression is greater in magnitude than the conduct coefficient of my primary institutional grant aid regression. Effective Institutional Price is simply cost of attendance (i.e. "list price") minus institutional grant aid. Yet the Challenged Conduct could have softened competition among Defendants on list prices via information sharing, or it could have affected packaging via the affordability principle; either of those mechanisms would raise the Effective Institutional Price without altering the EFC.[49] An alleged purpose of the cartel was to reduce competition on aid to reduce endowment spending and to reduce aid variability across members.[50] If increasing EFCs were the only mechanism to accomplish this goal, members could simply circumvent the cartel by altering another pricing element. For example, if a member school

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[49]. *See*, *e.g.*, Singer Report ¶¶188-191 (describing the effect of the affordability principle on packaging). *See also, e.g.,* Willem H. Boshoff and Johannes Paha, *List Price Collusion*, 21 JOURNAL OF INDUSTRY, COMPETITION AND TRADE, 393-409, 406 (2021) ("Inspired by recent evidence on list price collusion, we have reviewed several competition cases which suggest that agreements on list prices without coordinating on discounts are quite prevalent in the USA and Europe."); *id*. at 407 ("[C]ourts typically presumed that pure list price collusion would still be effective in raising final transaction prices, often relying on behavioral hypotheses: the courts either viewed *list prices serving as starting points or anchors in buyers' negotiations* with sellers or hypothesized that *list prices might serve as focal points facilitating firms' coordination on a collusive equilibrium at supracompetitive prices.*") (emphasis added); *id*. at 407 ("Our findings support the position of courts that list price collusion may be effective in producing higher prices even if there is no coordination on discounts and if the final prices are bargained bilaterally between sellers and buyers."); *In re High Fructose Corn Syrup Antitrust Litigation*, 295 F.3d 651, 656 (7th Cir. 2002) ("An agreement to fix list prices is, as the defendants' able counsel reluctantly conceded at the argument of the appeal, a per se violation of the Sherman Act even if most or for that matter all transactions occur at lower prices. Anyway sellers would not bother to fix list prices if they thought there would be no effect on transaction prices.").

[50]. Singer Report ¶3. Complaint ¶233; Bulman Report ¶9.

increased EFCs in coordination with other members, but offered increased grant aid to offset the

higher EFC, it could undercut the cartel's goal.

32.   Furthermore, Dr. Hill bases his opinion that my alternative models produce

"conflicting" results almost entirely on the statistical significance of my alternative model results

using his flawed two-way clustered standard errors.[51] As stated above and in my Rebuttal Report, I

disagree with Dr. Hill's assessment that statistical significance is the end-all be-all, and I disagree

with his replacement of my Huber-White robust standard errors with his two-way clustered standard

errors.

2.   My Impact Regressions Are Conservative for Myriad Reasons

33.   Dr. Hill misconstrues what renders an estimate "conservative." He argues that,

because my primary regression model produces damages estimates that are often higher than my

alternative specifications, this aspect purportedly means that my primary model is not

conservative.[52] I did not choose my primary regression model because it produces higher damages—

I chose it because it produced the best estimate of the conduct coefficient. Put differently, my

primary regression model measures the effect of the Challenged Conduct on the variable of interest

(Effective Institutional Prices), and it controls for the greatest number of potential confounders

through its inclusion of student controls, institutional controls, macroeconomic controls, and

student-Defendant fixed effects. I also implement the regression linearly, which, in the instant case,

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51. Hill Surrebuttal Report ¶32 ("For example, row one shows that models 1 and 2 in Dr. Singer's baseline methodology do not produce statistically significant estimated damages, while models 3 through 6 do."); *id.* ¶33 ("It also shows that the overcharge estimate in model 4 moves from being significant at the 5 percent significance level to being significant at only the 10 percent significance level."); *id.* ¶34 ("Rows three and four show that when using institutional grant aid instead of effective institutional price only 25 percent of Dr. Singer's overcharge estimates (i.e., three out of the twelve results across the two rows) are statistically significant at the 5 percent significance level."); *id.* ¶35 ("Rows five and six show that when using a log-linear form, Dr. Singer's model 3 loses statistical significance at the 10 percent significance level, which means that half of his models find no statistically significant evidence that the challenged conduct harmed students."); *id.* at n. 39 ("When I refer to the statistical significance of estimated damages, I am referring to the statistical significance of the underlying overcharge estimates.").

52. *Id.* ¶37.

can be viewed as the most reasonable functional form given the nature of how Effective Institutional Prices are set.[53] I explained this point in my Rebuttal Report, stating that universities tend to apply linear formulas in computing their need analysis methodology.[54]

34. My impact regressions are instead conservative because of the underlying data generating process, not because of their results.

35. *First*, my overcharges regressions give full credit to "leavers" by treating any Defendant that had ceased to participate as having fully disengaged in the Challenged Conduct. Record evidence suggests that Rice, for instance, left the 568 Group primarily to avoid paying membership dues, but continued to meet with 568 Group participants in COFHE and CNAR meetings during the years that I treat them as having "left."[55] Such an occurrence would lead to an underestimation of the true effect of the Challenged Conduct on Effective Institutional Prices because Rice's Effective Institutional Prices would still be at least somewhat affected by the Challenged Conduct even during the period I am treating as the benchmark.

36. *Second*, the umbrella effect likely produces an underestimation of the true overcharge, since Effective Institutional Prices throughout the Elite Private University Market generally would have been artificially inflated by the Challenged Conduct, even for years where a Defendant had not participated in the 568 Group.[56] For instance, my regressions count Rice as having not participated in the 568 Group from 2012 to 2014. But Rice having left over this period while other Defendants continued to engage in the Challenged Conduct would likely understate the degree of competition in the but-for world. In the but-for world, there would have been no 568 Group in the

53. Singer Rebuttal Report at n. 302.
54. *Id*.
55. *Id*. at n. 253.
56. *Id*. ¶73.

first place, which therefore would have promoted competition between these schools over the two decades in which the 568 Group had persisted.

37. *Third*, the economic literature states that there is often a lag between when a cartel ends and when market prices return to competitive levels, and this price reduction lag is greater the longer the cartel was in place.[57] Here, the alleged cartel lasted almost two decades.

38. *Fourth*, as Dr. Hill explains in his Initial Report, lock-in for first year students would imply that Effective Institutional Prices for first-years are "more informative about the impact of the alleged conduct (since those students are not locked in) than information in subsequent years."[58] As I explained in my Rebuttal Report, this argument would imply that my use of student-fixed effects regressions would underestimate the effect of the Challenged Conduct on Effective Institutional Prices.[59]

39. *Fifth*, as explained in my Rebuttal Report: "To the extent that previous membership in the [alleged] conspiracy reduced a Defendant's financial aid generosity subsequent to its departure from the 568 Group (just as antibodies from a previous infection or a previous vaccine might continue to offer protection after the expected cessation of said protection), treating such a Defendant as a 'clean' benchmark would understate the effect of the conspiracy."[60]

40. *Sixth*, as I explained in my Rebuttal Report, if any element of the Challenged Conduct was present both before and during the Class Period, as Dr. Stiroh claims in her Initial

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57. *Id.* at n. 188 (citing Joseph E. Harrington, Jr., *Post-Cartel Pricing During Litigation*, 52(4) THE JOURNAL OF INDUSTRIAL ECONOMICS 517-533, 517 (2004)).
58. Hill Report ¶228.
59. Singer Rebuttal Report ¶194. Dr. Hill claims that my student-fixed effects regressions "primarily rely on price changes for students that are locked in." *See* Hill Report ¶228. Applying Dr. Hill's own arguments, lock-in would mean that first-years are more informative as to the effect of the Challenged Conduct on Effective Institutional Prices, and, according to Dr. Hill, my student-fixed effects regressions primarily rely on non-locked-in students. Therefore, my overcharge estimates are conservative.
60. Singer Rebuttal Report ¶203.

Report,[61] this would serve to dilute the estimated effect of the Challenged Conduct on Effective Institutional Prices and make it harder for my analyses to find a statistically significant effect.[62] It is thus all the more remarkable that I did indeed find such an effect.

D.   Dr. Hill's "Sensitivity Tests" Amount to Improper "Data Dredging"

41.   In Dr. Hill's sur-rebuttal, he repeats his purported "sensitivity test" arguments that he had already brought up in his Initial Report.[63] His reiteration of these critiques appears to be improper for a sur-rebuttal report as the arguments are not responsive to any new empiricism. I will, nevertheless, briefly again explain why these sensitivity tests are wrong. Dr. Hill suggests that these sensitivity tests show that my models are unreliable because they alter my results; in reality, Dr. Hill's sensitivity tests alter my results because they improperly impair the ability of my model to measure the effect of the Challenged Conduct on Effective Institutional Prices.

42.   I concur with the common practice of conducting sensitivity tests to examine the robustness of results to alternative, economically justifiable, assumptions. I performed such tests in both my Initial and Rebuttal Reports. However, a distinction exists between sensitivity testing on the one hand and "data dredging," on the other, the latter meaning combing the data in search of results that one finds favorable. Often used synonymously with terms such as p-hacking, data mining, data snooping, and HARKing, data dredging represents the process of arbitrarily combing through data in an effort to obtain favorable results.[64] While such "favorable results" generally mean statistically significant ones, the reverse also applies. Here, under the guise of conducting "sensitivity tests," Dr. Hill engages in various attempts to overturn my results. In my Rebuttal

61. Stiroh Report ¶¶184-190.
62. Singer Rebuttal Report ¶¶206-210.
63. Hill Report §5.
64. HARKing means "Hypothesizing After Results are Known," *i.e.*, obtaining favorable results *then* manufacturing a theory to support them and *then* presenting it as an a priori theory that the results tested. *See*, *e.g.*, Norbert L. Kerr, *HARKing: Hypothesizing After the Results are Known*, 2(3) PERSONALITY AND SOCIAL PSYCHOLOGY REVIEW 196-217 (1998).

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Report, I explained the flaws in Dr. Hill's proposed "sensitivity tests."[65] To wit, he conflates tests for robustness of results with experimenting with regression model variants in an attempt to overturn results he finds unpalatable.

43.　　Dr. Hill applies what he terms "small and reasonable changes" to the regression models I estimated and discussed in my Initial and Rebuttal Reports.[66] Dr. Hill's changes are neither. For example, introducing a variable that has a spurious correlation with the treatment variable of interest, such as his inclusion of separate year dummies for every year, or for just 2020-2023 specifically (which he argues flexibly control for the effects of COVID), may represent a "small" change in his opinion, but (1) it is hardly reasonable, and (2) only serves to artificially absorb explanatory power properly attributed to the conduct coefficient.

44.　　More specifically, Dr. Hill presents three sensitivity tests. *First*, he replaces my use of a linear time trend with what he refers to as a "flexible" time trend.[67] Hill's approach amounts to adding a dummy variable for every year in the data, or what economists refer to as including time fixed effects. As stated in my Rebuttal Report, using time fixed effects here is problematic because many Defendants provided data that contain only one to two years in which the Defendant did not participate in the conspiracy (i.e., 2023 and 2024). But the data I use are annual, meaning that the year dummy variables are highly collinear with the Conduct variable. As a result of these facts, this data issue inhibits my model's ability to identify the effect of the Challenged Conduct on schools that were only out of the alleged conspiracy in 2023.[68]

45.　　*Second*, Dr. Hill replaces my use of a single dummy variable for COVID in 2020 with separate year dummy variables for 2020, 2021, 2022, and 2023. He asserts these variables

65. Singer Rebuttal Report §§III.A.2.d-e.
66. Hill Surrebuttal Report ¶45.
67. *Id.*
68. Singer Rebuttal Report ¶179.

control for COVID more "flexibly."[69] But this results in the same issue as noted above with regard to the time fixed effects—here, he merely includes the same dummy variables but for 2020-2023 instead of for every year in the data. This conveniently (for Dr. Hill) still produces a high degree of collinearity between when the end of the Challenged Conduct and the year dummy variables.

46. *Third*, Dr. Hill drops what he deems as "flawed" post-2022 data from all Defendants.[70] This is highly problematic because Defendants' nearly two-decade alleged conspiracy has imposed data limitations, rendering any benchmark data particularly valuable. Dr. Hill ignores this point, and chooses to drop significant data for when *all* Defendants had left the 568 Group.[71]

47. Given the nonexistent or minimal economic reasoning justifying his modifications to my regression specification, I conclude that his "small and reasonable changes" represent little more than thinly veiled attempts at data dredging or data mining.[72]

E. Dr. Hill's Critiques of My EFC Conduct Regressions Are Baseless

48. In Table 1 of my Rebuttal Report, in response to Defendants' Experts' claims that Defendants had not consistently implemented or reached consensus towards using the CM EFC,[73] I

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69. Hill Surrebuttal Report ¶45.
70. *Id*. ¶45.
71. Singer Rebuttal Report §III.A.2.e.
72. Jeffrey D. Michler, William A. Masters, and Anna Josephson, *Beyond the IRB: Towards a typology of research ethics in applied economics*, ALLIED SOCIAL SCIENCES ASSOCIATION, Invited paper presented at the 2019 Annual Meeting of the ASSA (Jan. 4-6, 2019) ("Once data has been collected, the researcher and her team must engage in analysis, with the goal of testing the hypothesis laid out in the project development stage. Here the primary unethical activity is p-hacking or data dredging, which is the practice of combing through data to uncover patterns that can be presented as statistically significant, without first devising a specific hypothesis as to the underlying causality."). *See also* WOOLDRIDGE at 685 ("Even if you are very careful in devising your topic, postulating your model, collecting your data, and carrying out the econometrics, it is quite possible that you will obtain puzzling results—at least some of the time. When that happens, the natural inclination is to try different models, different estimation techniques, or perhaps different subsets of data until the results correspond more closely to what was expected. Virtually all applied researchers search over various models before finding the 'best' model. Unfortunately, this practice of data mining violates the assumptions we have made in our econometric analysis.").
73. *See* Singer Rebuttal Report ¶37 (citing Stiroh Report ¶86, ¶150; Hill Report ¶¶113-114; Long Report ¶224, ¶239).

presented regressions demonstrating that the Challenged Conduct resulted in an approximately $515 artificial inflation in Class Member EFCs.

49.   Dr. Hill claims that my EFC conduct regressions—that is, my regressions where the dependent variable is EFC and independent variable is the conduct variable—are flawed for the same reasons that he purports that my Effective Institutional Price regressions are flawed. He is wrong here too. Specifically, he asserts that I did not use proper, two-way clustered standard errors, and he asserts that my EFC regression results change materially when applying either my sensitivity test or when applying his sensitivity tests.[74] All of the standard error rejoinders that I provide in both my Rebuttal Report[75] and in Part I.B above are directly responsive to Dr. Hill's flawed standard error critiques, and I therefore do not restate them again here. Similarly, the rejoinders to Dr. Hill's "sensitivity analyses" noted in my Rebuttal Report and in Parts I.C and I.D above are directly responsive to Dr. Hill's sensitivity critiques. I therefore do not restate them again here, with the exception of one additional point that Dr. Hill focuses on with regard to my alternative models applied to my EFC conduct regression.

50.   Dr. Hill notes that I do not present the results of my alternative log-linear specification applied to my EFC conduct regressions.[76] When he runs a log-linear version of my primary EFC conduct regression, he finds that the conduct coefficient is approximately zero.[77] In my Rebuttal Report, I had explained the reason for why the log-linear specification is not a proper alternative specifically for the EFC conduct regression.[78] The specification of EFC formulae is

74. Hill Surrebuttal Report §6.
75. Singer Rebuttal Report §III.A.2.b.
76. Hill Surrebuttal Report ¶61.
77. *Id.* ¶61.
78. Singer Rebuttal Report at n. 126.

known to be linear *a priori*.[79] For instance, EFC calculations use what are known as "assessment rates," which can be thought of as equivalent to a regression coefficient of a level variable. For instance, one might be assessed at 50 percent of their income, meaning that for each additional dollar of family income, EFC increases by \$0.50. Such a relationship can be written via the formula: $EFC = 0.5 * Income$.[80] Because this formula is known to be linear, implementing it via a logarithmic specification means that the model is misspecified.[81] Hence, for this reason, I only produced results of my EFC conduct regressions specified in levels to avoid bias.

F. Dr. Stiroh's Defendant-Specific EFC Conduct Regressions Share the Same Flaws as Her Defendant-Specific Effective Institutional Price Regressions

51. Dr. Stiroh recycles the same argument about splitting up my EFC conduct regressions as she had made for my Effective Institutional Price regressions in her Initial Report.[82] To address this alleged defect in my EFC conduct regressions, Dr. Stiroh tests splitting up the conduct variable in my EFC conduct regressions by Defendant and by time period, and she claims that doing so causes

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79. For example, consider the FM EFC. EFC formulae are broken down into several worksheets, each applicable based on a student's dependency status. The worksheets use addition, subtraction, multiplication, and division of a household or family's income, assets, and other relevant financial factors to compute the EFC. In their determination of the EFC, the worksheets notably do not feature the use of any non-linear computations, such as logarithms. *See The EFC Formula, 2023-2024*, FEDERAL STUDENT AID, https://fsapartners.ed.gov/sites/default/files/2022-08/2324EFCFormulaGuide.pdf (last visited Nov. 2024).

80. For example, a dependent student's contributions are determined by FM EFC Formula A. In this formula, the EFC is the summation of the available income of the student (multiplied by an assessment rate of 0.5), the available income of the parent contributions (not multiplied by any assessment rate), and the student contribution from their assets (multiplied by an assessment rate of 0.2). *See The EFC Formula, 2023-2024*, FEDERAL STUDENT AID, https://fsapartners.ed.gov/sites/default/files/2022-08/2324EFCFormulaGuide.pdf (last visited Nov. 2024) at 9-10.

81. *See*, *e.g.*, Kao-Lee Liaw, Myroslvana Khomik, and M. Altaf Arain, *Explaining the Shortcomings of Log-Transforming the Dependent Variable in Regression Models and Recommending a Better Alternative: Evidence from Soil CO$_2$ Emission Studies*, JOURNAL OF GEOPHYSICAL RESEARCH (May 7, 2021) ("Log-transforming the dependent variable of a regression model, though convenient and frequently used, is accompanied by an under-prediction problem. We found that this underprediction can reach up to 20%, which is significant in studies that aim to estimate annual budgets. The fundamental reason for this problem is simply that the log-function is concave, and it has nothing to do with whether the dependent variable has a log-normal distribution or not."). *See also* Robert Soczewica, *When should we use the log-linear model?*, TOWARDS DATA SCIENCE (Jan. 26, 2021), https://towardsdatascience.com/when-should-we-use-the-log-linear-model-db76c405b97e ("Thus we see that in practice we should use a log-linear model when dependent and independent variables have lognormal distributions. On the other hand, when those variables are normal or close to normal, we should rather stay with a simple linear model.").

82. Stiroh Surrebuttal Report ¶8.

the conduct coefficient to no longer show a consistent EFC overcharge for all Defendants, nor for all time periods.[83]

52.   In my Rebuttal Report, I enumerated multiple arguments for why Dr. Stiroh's splitting of my conduct variable by Defendant and by time-period is flawed when applied to my Effective Institutional Price regressions.[84] Those same arguments apply here. Namely, Dr. Stiroh's decision to split up the conduct variable separately by Defendant results in her excluding significant portions of the available, relevant data from Defendants, meaning that she excludes essential information that informs market effects on EFCs.[85] Nine out of 17 Defendants did not produce data prior to the start of the Challenged Conduct, making it much less likely that one can measure the effect of the Challenged Conduct on these Defendants' EFCs when they are separated out rather than when using a single conduct variable in a pooled regression, as I do.[86] She also ignores the fact that five Defendants only produced data covering a single academic year not subject to the Challenged Conduct, meaning that her Defendant specific EFC regressions for these Defendants assume that a single year after the Challenged Conduct ended can act as a reliable benchmark period.[87] Dr. Stiroh provides no reasonable *a priori* justification for how she chooses to split up the conduct variable by time-period, rendering her time-period specific results invalid. For instance, she claims to use 2015–2019 as one of the time periods because during that period "Chicago left the 568 Group and Yale rejoined the 568 Group."[88] But based on her own logic, she should have instead split this time-period into two time periods, 2015-2017 and 2018-2019, since Chicago purportedly left in 2015, and Yale

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83. *Id.* ¶10, ¶13.
84. Singer Rebuttal Report §III.A.2.a.
85. *Id.* ¶¶75-76.
86. *Id.* ¶79.
87. *Id.* ¶80.
88. Stiroh Surrebuttal Report at n. 401.

rejoined starting in 2018.[89] The econometrics literature advises against imposing arbitrary breaks such as what Dr. Stiroh proposes without having a strong *a priori* basis for doing so.[90] Like Dr. Hill, Dr. Stiroh cannot redefine the Challenged Conduct. Plaintiffs allege a single overarching conspiracy across all Defendants. Hence, the appropriate model to test is whether the Challenged Conduct inflated Effective Institutional Prices across all Defendants jointly, not separately.

II. DEFENDANTS' EXPERTS' ADDITIONAL CRITIQUES REGARDING CLASSWIDE IMPACT ARE UNAVAILING

53.   The second step of my two-step approach to demonstrating common impact involves showing that the generalized overcharge calculated in the first stage translated into artificially inflated Effective Institutional Prices for all or nearly all Class Members. I presented three distinct quantitative analyses that support classwide impact—(1) in-sample prediction, (2) price-structure regressions, and (3) a common shock analysis. I also presented qualitative evidence of common impact, including both economic theory (explaining how the goal of horizontal and vertical equity would have resulted in widespread effects) and record evidence.[91] Dr. Stiroh presents further critiques of the second step of my two-step approach in her sur-rebuttal, largely repeating the same points that she had already discussed in her Initial Report (or making points that she could have made in her Initial Report but failed to do). I respond to each of her arguments below.

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89. Singer Rebuttal Report ¶86; Stiroh Report ¶¶175-179; Stiroh Report at n. 401.

90. *See*, *e.g.*, Bruce E. Hansen, *The New Econometrics of Structural Change: Dating Breaks in U.S. Labor Productivity*, 15(4) JOURNAL OF ECONOMIC PERSPECTIVES 117-128, 118 (2001) ("The classical test for structural change is typically attributed to Chow (1960). His famous testing procedure splits the sample into two subperiods, estimates the parameters for each subperiod, and then tests the equality of the two sets of parameters using a classic F statistic . . . However, an important limitation of the Chow test is that the breakdate must be known *a priori*. A researcher has only two choices: to pick an arbitrary candidate breakdate or to pick a breakdate based on some known feature of the data. In the first case, the Chow test may be uninformative, as the true breakdate can be missed. In the second case, the Chow test can be misleading, as the candidate breakdate is endogenous—it is correlated with the data—and the test is likely to indicate a break falsely when none in fact exists. Furthermore, since the results can be highly sensitive to these arbitrary choices, different researchers can easily reach quite distinct conclusions—hardly an example of sound scientific practice.").

91. Singer Report §III.B.

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A.   Dr. Stiroh's Assertions That My EFC Conduct Regression and Variance Analyses Do Not Show Classwide Impact Are Irrelevant

54.   Dr. Stiroh claims that my EFC Conduct regressions do not, by themselves, measure whether the EFCs for all or nearly all Class Members were inflated by the Challenged Conduct.[92] This argument is irrelevant. I already evaluated classwide impact for Effective Institutional Prices using, among other analyses and evidence, in-sample prediction, price-structure regressions, and a common shock analysis.[93] As explained above, the EFC conduct regressions demonstrate one mechanism (of several possible mechanisms) used to increase the Effective Institutional Prices, and thus bolster a portion of my common impact analysis.

55.   In my Rebuttal Report, I specified EFC and Effective Institutional Price variance regressions.[94] These showed that the Challenged Conduct is associated with lower variation in EFCs and Effective Institutional Prices between Defendants. Dr. Stiroh in her sur-rebuttal asserts that these analyses of mine "say[] nothing about whether offers were higher or lower or even whether offers were closer together for all of substantially all proposed Class members."[95] This argument misconstrues these analyses. Because EFCs and Effective Institutional Prices are being compared for the same student at two different Defendants, only students who were admitted to at least two Defendants in the same year are included in this analysis. Not every student is admitted to multiple Defendants, so naturally this analysis will not provide specific evidence encompassing every Class Member. The point, however, is that the reduced variance of EFCs for cross-admits is indicative of an anticompetitive effect flowing from the Challenged Conduct—upon which all Class Members can rely to bolster their case. After showing a generalized effect on Effective Institutional Prices in

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92. Stiroh Surrebuttal Report ¶7.
93. Singer Report §III.B.
94. Singer Rebuttal Report Table 2 and Table 3.
95. Stiroh Surrebuttal Report ¶14.

my impact regression, I demonstrate common impact with, among other evidence and analyses, my in-sample predictions, price-structure regressions, and a common shock analysis in my Initial Report.[96] The EFC and Effective Institutional Price variance regressions show that, when two Defendants were members of the 568 Group, the EFCs and Effective Institutional Prices offered to admitted students were closer in value than the EFCs and Effective Institutional Prices offered to students admitted to Defendants who were not both members of the 568 Group in a given year.

B.   Contrary to Dr. Stiroh's Claims, I Establish That Packaging Is a Mechanism for Classwide Impact

56.   Dr. Stiroh claims that I have offered "no explanation as to what any supposed agreement on packaging entailed."[97] This is false. In my Initial Report, I explained that packaging would likely be affected by the Challenged Conduct, due to, among other of its elements, the common adoption of the affordability principle.[98] Packaging under the alleged Overarching Agreement was ultimately determined based on a family's ability to pay, including the capacity to take out loans.[99] I provided qualitative evidence of this effect, including the rationale for capping home equity, as doing so would "reflect the reasonable borrowing capacity of families who choose to use the equity in their homes as a source of capital for funding their child's education."[100] I provided record evidence from Notre Dame showing that it considered a student's capacity to pay back any need-based loans.[101] In my Rebuttal Report, I provided additional record evidence from Emory suggesting that Emory considered withdrawing from the 568 Group because doing so would

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96.   Singer Report §III.B.
97.   Stiroh Surrebuttal Report ¶26.
98.   Singer Report ¶¶188-191.
99.   *Id*. ¶188.
100.   *Id*. ¶190.
101.   *Id*. ¶191.

"give Emory far greater flexibility in the packaging of financial aid."[102] The same logic of the Challenged Conduct would lead to restrictions on packaging; if Defendants could compete under the alleged cartel using a non-price-fixed element, then one of the main points of the alleged cartel (suppressing competition over aid/price and reducing variability across schools) would be undermined.

57.   Synthesizing the results from my Effective Institutional Price regressions and EFC conduct regressions together, I find that these results combined provide quantitative evidence that the Challenged Conduct had affected packaging. I calculate that the Challenged Conduct resulted in an artificial overcharge in EFCs of roughly $515, and an artificial overcharge in Effective Institutional Prices of $1,202.[103] This differential between the conduct coefficients of my Effective Institutional Price and EFC conduct regressions can be thought of as measuring the effect of the Challenged Conduct on (1) packaging via a shift from institutional grant aid to loans or (2) list prices via information sharing or both.

58.   Dr. Stiroh presents a packaging analysis in Figure S3.7 of her sur-rebuttal report, showing that total grants cover all estimated financial need, calculated as the cost of attendance minus EFC, for more than ten percent of Class Members.[104] She claims that for these Class Member observations, "packaging in the but-for world could not be better than packaging in the actual world."[105] I do not dispute Dr. Stiroh's claim that *some* Class Members may not have been impacted *via* packaging. But this analysis is uninformative as to whether these Class Members were impacted

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102.   Singer Rebuttal Report ¶40; *id.* at n. 118 (citing Emory_568Lit_0006213). *See also* Emory_568Lit_0006214 (in purportedly leaving the 568 Group, in 2012, Emory officials lamented that the Group "generates a restrictive environment for packaging financial aid at a time when college and universities need to be more flexible and responsive[.]").
103.   *See* Singer Rebuttal Report Table 1 Column 6; *id.* Table 6 Column 6.
104.   *See* Stiroh Surrebuttal Report ¶27.
105.   *Id.* ¶27.

in any form from the Challenged Conduct—specifically, for the ten percent of Class Members that Dr. Stiroh flags. These students must have a positive EFC in order to still be counted as part of the Class. Therefore, based on my EFC conduct regressions, these Class Members would still be expected to have been impacted via artificially inflated EFCs even though grants may have covered the remaining portion of their estimated financial need. And that is what my common impact analysis shows—namely, that all or nearly all members of the Class paid higher prices due to the Challenged Conduct (whether that was from reduced competition over packaging or EFCs).

C.   Dr. Stiroh's Critiques of My "Harmed on Net" In-Sample Analysis Do Not Refute Classwide Impact

59.   In Dr. Stiroh's Initial Report, she claimed that my in-sample prediction methodology "is fundamentally flawed and econometrically invalid."[106] To the contrary, my in-sample prediction is a standard methodology employed by economists to show how a generalized effect translates into widespread impact.[107] This method first involves running a regression model to measure the effect of some variable of interest—in this case, the effect of the Challenged Conduct on Effective Institutional Prices. It then involves using this regression model to predict what Effective Institutional Prices would have been absent the Challenged Conduct, and testing whether actual Effective Institutional Prices were higher than but-for Effective Institutional Prices. When the actual Effective Institutional Price for a particular student in a particular academic year exceeds the but-for

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106.   Stiroh Report ¶197.

107.   In-sample prediction is based upon the Federal Judicial Center's *Reference Manual on Multiple Regression,* and it has been cited in several recent antitrust cases wherein economists have used this same in-sample prediction to demonstrate common impact. *See* REFERENCE MANUAL at 308. *See also In Re Broiler Chicken Growing Antitrust Litigation (No. II)*, 6:20-MD-02977-RJS-CMR (E.D. Ok May 8, 2024) (Memorandum Decision and Order Granting Plaintiffs' Motion for Class Certification and Denying Defendant's Motion to Exclude) at 40 ("The in-sample prediction method is a standard technique used to test whether the impact of an antitrust conspiracy is widespread."); *id*. at n. 242 (citing *Olean Wholesale Grocery Coop., Inc. v. Bumble Bee Foods*, LLC, 31 F.4th 651, 672 (9th Cir. 2022); *In re Capacitors Antitrust Litig.* (No. III), Case No. 17-md-02801, 2018 WL 5980139, at *7–9 (N.D. Cal. Nov. 14, 2018); *In re Domestic Drywall Antitrust Litig.*, 322 F.R.D. 188, 217 (E.D. Pa. 2017); *In re Korean Ramen Antitrust Litig.*, Case No. 13-cv-04115, 2017 WL 235052, at *6 (N.D. Cal. Jan. 19, 2017); *In Re Broiler Chicken Antitrust Litigation*, Case No. 16-cv-8637, 2022 WL 1720468, at *10, *13 (N.D. III. May 27, 2022).

price for that transaction, that is an indication that the Class Member was overcharged on that transaction.

60. In part, Dr. Stiroh's argument is based on her claim that in-sample prediction could lead to false positives. She is wrong. More specifically, Dr. Stiroh asserts that my in-sample prediction methodology would show a high share of Class Members having been "impacted" even if the estimated overcharge were equal to zero.[108] I responded to this critique in my Rebuttal Report, among other ways, by pointing out that in-sample prediction is a standard method used for this very purpose. I also explained that Dr. Stiroh's argument is predicated on a false premise—that the generalized overcharge is zero.[109] If the generalized overcharge were zero, the analysis would stop there. The second step in the in-sample method is only to be used if the first step demonstrates that the Challenged Conduct had a statistically significant effect on price. If one goes to the second step, when there truly is no effect at all from the Challenged Conduct, the method will not tell us anything useful. As discussed below, I went beyond the first step here because it showed a statistically significant relationship between the Challenged Conduct and artificially inflated prices to Class Members. As a result, the second step is capable of showing that the share of Class Members impacted is signal, not merely noise. To summarize, the in-sample prediction analysis presented in my Initial Report and updated in my Rebuttal Report backup showed that over 96 percent of Class Members had paid artificially inflated Effective Institutional Prices during at least one academic year during the Class Period.[110] This, combined with the other forms of classwide evidence, establishes common impact from the Challenged Conduct.

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108. *Id*. ¶198.
109. Singer Rebuttal Report ¶¶227-233.
110. Singer Report Table 12; Singer Rebuttal Report workpapers.

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61.   That my in-sample prediction shows widespread impact across the Class is impressive for two reasons. *First*, my impact regression, as discussed above, is highly conservative because the benchmark period available to me in the data likely contained transactions that continued to be at least partially affected by the Challenged Conduct.[111] *Second*, as discussed below, the in-sample method itself is a highly conservative method. This is because in-sample prediction assumes that any downward shock to prices in the actual world would vanish in the absence of the Challenged Conduct. For illustrative purposes, consider a student that happened to have received a significant one-off financial aid award due to some unexpected change in her financial or family circumstances, and that this reduced her Effective Institutional Price substantially during her first academic year.[112] For instance, suppose that this student paid Effective Institutional Prices of $9,000 in 2009, $41,000 in 2010, $39,000 in 2011, and $30,000 in 2012. Because of the unpredictability of this student's price in 2009 compared to her other years, my in-sample prediction would be unlikely to find harm during that year, because the predicted but-for price would likely be greater than the actual price of $9,000, merely due to the unpredictability of the student's change in circumstances. This result does not mean, however, that the student's actual price was unaffected by the Challenged Conduct. There is no basis to presume that this discount would not have also occurred in the but-for world—rather, one would expect that this student would have paid an *even lower* price due to the competitive pressures that otherwise have been dampened by the alleged conspiracy. The nature of the in sample

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111.   *See* Part I.D, *supra*.

112.   Note that I use the terminology "unexpected" here to refer to a change in the student's circumstances that the model treats as being the result of random noise. While my impact regressions control for myriad factors—income, net worth, number in family attending college, the Defendant's endowment, to name a few—the purpose of a model is not to *perfectly* predict prices, it is to reliably measure the artificial overcharge in Effective Institutional Price resulting from the Challenged Conduct. *See*, *e.g.*, Amy Gallo, *A Refresher on Regression Analysis*, HARVARD BUSINESS REVIEW (2015), https://hbr.org/2015/11/a-refresher-on-regression-analysis (last visited Nov. 8, 2024) ("A regression line always has an error term because, in real life, independent variables are never perfect predictors of the dependent variables.").

analysis, however, would nevertheless treat this transaction as not being impacted, even where it

likely was.

62.   Indeed, Dr. Stiroh herself acknowledges the conservative nature of in sample

methodology in a 2016 ABA article she wrote referencing the use of in-sample prediction in the

Electronic Books case.[113] In that case, the defense expert used in-sample prediction to try and refute

common impact. As Dr. Stiroh stated:

> This example shows that, when comparing an average but-for price derived from regression
> results to the entire distribution of actual prices, it is possible that the average but-for price
> is higher than some actual prices. As the plaintiffs' expert demonstrated in this case, actual
> prices could be lower than the predicted average but-for price in the presence of discounts.
> However, **it does not necessarily imply that the consumers who paid those actual prices
> were unharmed by a conspiracy** under the plaintiffs' theory.[114]

By the same logic, my use of in-sample prediction is conservative because it necessarily implies that

students who paid actual prices even trivially below the but-for predicted price due to having

received an unexplained increase in institutional grant aid as being unharmed. That is conservative,

and understates impact because there is no reason to believe that these students would not have

received the same unexplained increases in institutional grant aid in the but-for world. In other

words, the in-sample method simply assumes that all reductions in Effective Institutional Price not

picked up by the variables in my model would have been due to the Challenged Conduct. And that

is a very conservative assumption, making it difficult to pick up injury. Even so, my implementation

of this very conservative method showed that over 96 percent of Class Members were injured.

63.   In my Rebuttal Report, I presented a variant of my original in-sample prediction

analysis referred to as a "harmed on net" analysis in support of my showing of common impact.[115]

This involved predicting Class Members' but-for Effective Institutional Prices using my impact

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113.   Christine Siegwarth Meyer, Lauren J. Stiroh, and Claire (Chunying) Xie, *Demonstrating Faulty Predictions in Class Certification Analysis*, 30(2) ABA ANTITRUST (2016).
114.   *Id.* at 9.
115.   Singer Rebuttal Report ¶¶234-235.

regression, but this time summing the difference between what each Class Member actually paid minus what they would have paid absent the Challenged Conduct across all academic years in which that Class Member was subject to the Challenged Conduct (i.e., was at a Defendant while that Defendant was allegedly actively participating in the 568 Group).[116] I explained that showing impact under this methodology requires an overcharge on net across multiple years, and thus it is even harder to demonstrate impact than my prior method (which required only a single year of impact for a given Class Member). Under this "on net" variation of the in-sample model, if there was in fact no overcharge of a student, positive and negative predicted differences between actual prices and but-for prices would net out when one sums these differences across a Class Member. Of course, this in-sample variation is conservative for all of the reasons discussed in my prior reports and above, and also for this additional reason. Thus, it is remarkable that it showed 95 percent of Class Members as having been harmed on net, which (given the conservative assumptions in the method) is further strong evidence that the Challenged Conduct had a classwide effect.[117]

64.   In Dr. Stiroh's sur-rebuttal report, she argues that my harmed on net analysis does not test for common impact, but instead imposes it.[118] In essence, she claims that there are two subsets of Class Members that are driving my results—Class Members who show up in the data exclusively during the period subject to the Challenged Conduct, and Class Members who only show up in the data for a single year.[119] She argues that my harmed on net analysis is mechanically guaranteed to show harm for these two Class Member subsets.

65.   Dr. Stiroh creates the impression that no Class Member in the two subsets she analyzes could have been injured by the Challenged Conduct because they are mechanically

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116. *Id.*
117. *Id.*
118. Stiroh Surrebuttal Report ¶16.
119. *Id.* ¶21.

guaranteed to show as being impacted by the in-sample methodology. In fact, Class Members who show up only during the Class Period, which comprise the majority of the two Class Member subsets Dr. Stiroh takes issue with, would in fact be *more* likely to have been injured than students who show up both inside and outside of the Class Period for two reasons.

66.   *First*, the students who have some years in the benchmark period and some years in the Class Period fall into two buckets, both of which are less likely to show up as being impacted under my in-sample methodology. The first is a collection of students that attended at the outset of when their institution had joined the 568 Group. In other words, these are students who were already attending their Defendant institution when their institution joined the alleged conspiracy. Economic theory recognizes that it can take time for the full impact of an alleged cartel to take effect. The effectiveness of cartels often can sharpen over time, as the members develop collective plans and strategies and enforcement mechanisms. So, for students at schools who joined during the 568 Group's formation or just for schools who joined late but would not necessarily have fully implemented all of the 568 Group's proposals and agreements, it would be harder to show impact because, as with many cartels the effects of an institution joining could take some time to fully express itself in pricing. The second bucket is Class Members attending Defendant institutions during a period before and after their institution left the 568 Group. For at least some of these Defendants, the umbrella effect (discussed above) might continue to artificially inflate Effective Institutional Prices for one or more years after that Defendant left. Thus, these two buckets of Class Members are the least likely to be impacted by the Challenged Conduct, yet these are the students that Dr. Stiroh suggests I should limit my in-sample analysis to.

67.   The *second* reason that the Class Members that Dr. Stiroh implies the data should be limited to would result in conservative in-sample results is because they have fewer years in which

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their school was in the 568 Group. Consider the circumstances that would result in a Class Member

escaping injury under the in-sample prediction test in a given academic year. For this to occur, it

would have to be the case that this student was awarded significantly higher institutional grant aid

to offset the generalized overcharge found via my regression model. For each year that a Class

Member is subject to the possibility of being overcharged as a result of the Challenged Conduct, the

more likely it is that said Class Member is injured. Class Members who only show up during the

Class Period have the greatest number of chances of being injured based on this logic—and these

are exactly the Class Members that Dr. Stiroh suggests I need to exclude from my test. Thus, if my

method finds that, all things equal, the students least likely to be injured are injured to a broad extent,

then it shows that those who would have borne the brunt of the Challenged Conduct are far more

likely to be injured.

68.   Furthermore, in column 3 of both my Effective Institutional Price and EFC conduct

regression tables in my Rebuttal Report, I presented an alternative overcharge model specification

that does not use student fixed effects, and I continue to find that the Challenged Conduct had

artificially inflated both Effective Institutional Prices and EFCs.[120] Because these models do not

include student fixed effects, they therefore allow all students to directly inform the overcharge,

including students who show up solely during the Class Period or who only show up in the data

once.[121] These results are therefore suggestive that these particular student groups had been subject

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120. Singer Rebuttal Report Tables 1 and 6. In column 3 of Table 6 in my Rebuttal Report, I estimate that the
Challenged Conduct had resulted in a $945 artificial overcharge in Effective Institutional Price when not including
student fixed effects, and in column 3 of Table 1 of my Rebuttal Report, I estimate that the Challenged Conduct had
resulted in a $753 artificial overcharge in EFCs when not including students fixed effects.

121. Note that I do not consider the non-student fixed effects models in columns 1-3 of my regression tables to be
my primary models for assessing overcharges because they do not control for student-specific time invariant factors that
could be correlated with the Challenged Conduct. Nevertheless, even with this fault, I find that the non-student fixed
effects analog of my primary regression model show that the Challenged Conduct had artificially inflated both Effective
Institutional Prices and EFCs.

to a generalized artificial overcharge, in contrast to Dr. Stiroh's implication that they had not been, otherwise the conduct coefficient from these regressions would be zero.[122]

69.   For these many reasons listed above, I disagree with Dr. Stiroh's implication that Class Members who exclusively show up during the Class Period or who only show up in the data once should be excluded from my measure of the proportion of Class Members impacted. These students were likely harmed by the conduct. I find that these Class Members were harmed, in part, because my model without student fixed effects, where the conduct coefficient is *largely* and *directly* informed by non-switchers, still shows an economically and statistically significant effect of the alleged conspiracy.

70.   Nevertheless, I test Dr. Stiroh's assertion that my findings are mechanically guaranteed by the two "problem" groups she identifies, and I find that she is wrong that these groups drive my findings. I analyze the particularly informative scenario of Yale, which had expressly stated that it left the 568 Group in 2008 in order to compete on pricing. When Yale departed from the 568 Group in 2008, then-Director of Student Financial Services, Caesar Storlazzi, stated "[b]y leaving the 568 Group, Yale is now free to give families more aid than they would have gotten under the consensus methodology."[123] Given the specifics of this statement, it provides a unique (albeit conservative) experiment for measuring what prices would have looked like in a proper but-for world under competition. It is still conservative given that while Yale says it left to be "free to give families more aid," that too would be likely to ramp up over time and not necessarily show up all at

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122. Additionally, these models are conservative because, for instance, they include several Defendants who had only produced data after the start of the Challenged Conduct. Caltech, Cornell, Georgetown, Johns Hopkins, MIT, Notre Dame, and Rice all produced financial aid data only covering years after 2003, when the Challenged Conduct began. Because we do not observe these Defendants' Effective Institutional Prices prior to the start of the Challenged Conduct, and because students that show up solely during the Class Period at these Defendant universities directly inform the conduct coefficient of my non-student fixed effects regressions, my non-student fixed effects regressions tend to produce conservative conduct coefficient estimates.

123. Caitlin Roman, *University Leaves Financial Aid Group*, YALE DAILY NEWS (Sept. 26, 2008), https://yaledailynews.com/blog/2008/09/26/university-leaves-financial-aid-group/.

once.[124]

71.   Because this analysis only includes Yale Class Members that show up in the data both before-and-after Yale chose to leave the 568 Group in 2008, it therefore is directly in response to the in-sample critiques Dr. Stiroh raises in her sur-rebuttal report. The Yale Class Members that I include in this analysis only show up both inside and outside of the Class Period, and therefore, by definition, these Class Members must show up in the data more than once. Dr. Stiroh takes issue with Class Members who either only show up in the Class Period or who only show up in the data once—this analysis excludes both of these groups. Additionally, note that Dr. Stiroh claims that "The fundamental issue with Dr. Singer's methodology is that any variation in outcomes unexplained by the regression model is conflated with the average effect, if any from the conduct. Indeed, in a well specified linear regression model, roughly half of the fitted values for a variable will be above the observed values and half will be below."[125] Dr. Stiroh admits that under a hypothetical scenario where the overcharge is zero, half of the *transactions* would show up as being harmed (i.e., having a higher actual price than the but-for predicted price). By transaction, I am referring to an observation in the data—that is, a specific Class Member-academic year instance. If one were to find that the share of *transactions* harmed is materially greater than 50 percent, this would be indicative that my method demonstrates that the overall effect of the Challenged Conduct on Effective Institutional Prices outweighs individual variation.

72.   I apply my in-sample prediction to the subset of Yale Class Member transactions discussed—that is, Class Members who show up in Yale's data before-and-after Yale had left the 568 Group in 2008. I first run my primary Effective Institutional Price regression (from my prior reports), and I then run my in-sample prediction method on the Yale 2008 before-and-after subset

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124.   *Id.*
125.   Stiroh Report ¶198.

of Class Members. My in-sample prediction method involves comparing each of these Class Member's actual Effective Institutional Prices with their predicted but-for Effective Institutional Prices, and counting up how many transactions show up with an actual Effective Institutional Price greater than the but-for Effective Institutional Price. I calculate that 81 percent of these transactions were impacted—that is, 81 percent of this subset of transactions occurring prior to Yale having left the 568 Group in 2008 show higher actual Effective Institutional Prices than the predicted but-for prices. This 81 percent result is *significantly* higher than the 50 percent benchmark that would be expected under a zero overcharge, and it therefore indicates that my model is identifying far more than noise, especially in light of the conservative nature of this analysis.

73. This test is highly conservative for multiple reasons. As stated above, in-sample prediction is a conservative methodology generally because it simply assumes that all reductions in price not picked up by the variables in my model are due to the Challenged Conduct. Moreover, as stated above, it would take time for the impacts of leaving the alleged cartel to fully express themselves in pricing, and yet this analysis necessarily compares a student's actual and but-for pricing during the period immediately before and after Yale's claimed departure from the 568 Group. Further, I do not measure impact by calculating the share of Class Members *ever* impacted at least once in this analysis, but I instead as the share of transactions that were impacted, which is more conservative.[126] In reality, if a Class Member attends a Defendant institution for multiple years, the probability of obtaining an actual Effective Institutional Price greater than the predicted but-for Effective Institutional Price increases due to the fact that they are exposed to the Challenged Conduct

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126. In my Initial Report, and in my Rebuttal Report backup, I computed the percent of Class Members that were ever impacted, which is calculated by counting whether a Class Member had paid a higher actual Effective Institutional Price than their but-for predicted Effective Institutional Price in any academic year in which they were subject to the Challenged Conduct. I calculated that over 96 percent of Class Members were impacted at least once. *See* Singer Rebuttal Report Workpapers; Singer Report Table 12.

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over more instances. This makes it far less likely that one would find a share of transactions impacted

that is higher than fifty percent, even if it were the case that all or nearly all Class Members were

impacted at least once, which is what I showed in my Initial Report.[127]

74.   It bears noting that Dr. Stiroh does not address my use of in-sample prediction to

compute the average percentage overcharge in Effective Institutional Prices by Defendant, which I

provided in my Initial Report.[128] In Table 3, I re-present this table, now using the updated overcharge

percentages calculated using my updated Effective Institutional Price regressions in my Rebuttal

Report. I had also provided this in my Rebuttal Report workpapers. I continue to find that each

Defendant had artificially inflated its Effective Institutional Prices as a result of the Challenged

Conduct, consistent with Class Members at every Defendant institution having been subject to

higher Effective Institutional Prices.

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127. Singer Report ¶¶258-259 and Table 12.
128. Singer Report Table 13.

TABLE 3: IN-SAMPLE ANALYSIS
EFFECTIVE INSTITUTIONAL PRICE OVERCHARGE PERCENTAGE BY DEFENDANT (USING ADJUSTED REBUTTAL REPORT DATA)

Defendant	Effective Institutional Price Overcharge (%)
Brown	4.4%
Caltech	2.4%
Chicago	3.4%
Columbia	5.7%
Cornell	4.9%
Dartmouth	5.3%
Duke	3.1%
Emory	3.8%
Georgetown	3.6%
Johns Hopkins	4.3%
MIT	6.5%
Northwestern	3.6%
Notre Dame	4.1%
Penn	4.8%
Rice	6.7%
Vanderbilt	5.2%
Yale	6.1%

Notes: Percentage values correspond to the average Effective Institutional Price overcharge across Class Members and academic years in the sample during the Class Period.

75.   It also important to note that Dr. Stiroh's sur-rebuttal arguments only apply to my in-sample prediction methodology but have no bearing on my price-structure regressions or on my common shock analysis, both of which I used to determine whether the generalized artificial overcharge resulted in widespread impact to all or nearly all Class Members.[129] Her arguments also have no bearing on the qualitative evidence presented in my Initial Report noting that universities consider both horizontal and vertical equity when awarding institutional grant aid, which is consistent with the existence of a pricing structure that would transmit a generalized artificial overcharge to all Class Members.[130]

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129. *Id.* §III.B.2.a.
130. *Id.* §III.B.2.b.

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III. DR. HILL'S CRITIQUES OF MY CROSS-ADMIT ANALYSIS ARE UNAVAILING

A.   **Dr. Hill's Changes to My EFC and Effective Institutional Price Variance Regression Methodology Make No Difference**

76.   In Dr. Hill's Initial Report, he presents various analyses purporting to show that Class Members who were cross-admitted to more than one Defendant (i.e., "cross-admits") received different EFCs and net prices from the different Defendants.[131] He claims that these results indicate that Defendants did not standardize EFCs or the subsequent prices.[132] In my Rebuttal Report, I explain that exact matches on EFCs or net prices are not necessary to indicate collusion on either EFCs or the Effective Institutional Price, especially given that the alleged conspiracy was designed to set a *floor* for EFCs.[133] I show that students admitted to two Defendants who were both in the 568 Group have EFCs and Effective Institutional Prices that are closer together than the EFCs and Effective Institutional Prices for students who were admitted to Defendants who were not both in the 568 Group at the time these students were admitted.[134] This result indicates that membership in the 568 Group reduced the variation between the financial aid offers that students admitted to multiple Defendants received. Such a finding is consistent with Plaintiffs' allegations that the alleged conspiracy reduced price competition between Defendants and inconsistent with unfettered competition.

77.   Dr. Hill claims to find three flaws with my cross-admit EFC and Effective Institutional Price analyses. First, he argues that the data are not adequate for my methodology because there is an uneven distribution of in-and-out-of-conduct data.[135] Next, Dr. Hill claims that given school pairs have systematic differences in EFC and Effective Institutional Price ranges

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131. *See*, *e.g.*, Hill Report Figure 14, Figure 20.
132. Hill Report ¶101, ¶120.
133. Singer Rebuttal Report ¶304, ¶307.
134. *Id.* Table 2, Table 3.
135. Hill Surrebuttal Report §7.1.

compared to other school pairs.[136] Finally, he opines that school pairs with one school as a member

of the 568 Group and one school not a member of the 568 Group ("mixed pairs") should be treated

as a separate group, rather than being considered a part of the non-conduct group.[137] Dr. Hill claims

to correct my methodology for each my EFC and Effective Institutional Price by adding school-pair

fixed effects and removing all mixed pairs from the data. He claims that these changes cause the

conduct to no longer be statistically significant.[138]

78.   Specifically, Dr. Hill makes two changes: (1) he removes the mixed pairs from the

data and (2) adds school-pair fixed effects. He also uses clustered standard errors both when

illustrating my results both with my amended data and with his amended data. I previously explained

both in my Rebuttal Report and above in this sur-reply that Dr. Hill's use of clustered standard error

is inapposite here.[139] Even before evaluating the merits of Dr. Hill's two methodological alterations,

simply using my amended data and robust ("Huber-White") standard errors shows that both the EFC

and Effective Institutional Price regression results remain negative, economically, and statistically

significant at the 5 percent level. Tables 3 and 4 below present these results.

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136. *Id.* §7.2.
137. *Id.* §7.3.
138. *Id.* §7.4.
139. *See* Part I.B, *supra*. *See also* Singer Rebuttal Report §III.A.2.b.

TABLE 3: RANGE IN EFCS FOR CROSS-ADMITTED STUDENTS

	Dependent Variable: *Real EFC Range*	
	(1)	(2)
Conduct	**-2,174.74***	**-2,164.40***
	(0.002)	**(0.001)**
Average Real Income		82.30***
		(0.000)
Average Real Net Worth		2.09
		(0.281)
Average Number in College		-4,502.03***
		(0.000)
Unemployment (1-year lag)		-172.44**
		(0.012)
Admitted Defendant Count		-961.47***
		(0.000)
Ranking Range		-121.15*
		(0.056)
COVID		-1,536.31**
		(0.043)
Trend		528.99***
		(0.000)
Real GDP		-212.39
		(0.573)
Observations	90,215	90,112
R-Squared	0.035	0.135

Notes: Robust p-values indicated as follows: ***p<0.01, **p<0.05, *p<0.1. Real GDP is measured in millions of dollars. Average income and average assets are measured in real dollars.

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TABLE 4: RANGE IN EFFECTIVE INSTITUTIONAL PRICES FOR CROSS-ADMITTED STUDENTS

	Dependent Variable: *Real Effective Institutional Price Range*	
	(1)	(2)
Conduct	**-1,685.46*****	**-1,257.63*****
	(0.000)	**(0.000)**
Average Real Income		-23.11***
		(0.000)
Average Real Net Worth		-0.25
		(0.241)
Number in College		1,045.73***
		(0.000)
Unemployment (1-year lag)		184.10***
		(0.000)
Admitted Defendant Count		385.70***
		(0.000)
Ranking Range		-276.84***
		(0.000)
COVID		-1,178.10***
		(0.003)
Trend		763.33***
		(0.000)
Real GDP		-530.84***
		(0.004)
Observations	84,672	84,593
R-Squared	0.213	0.261

Notes: Robust p-values indicated as follows: ***$p<0.01$, **$p<0.05$, *$p<0.1$. Real GDP is measured in millions of dollars. Average income and average assets are measured in real dollars.

79.   Tables 3 and 4 indicate that Dr. Hill's methodological changes make no material difference to my results. Rather, such differences result from his alternative data and standard error choices, both of which I reject. As explained in my original Rebuttal Report, Dr. Hill makes several unnecessary and inappropriate adjustments to my amended dataset.[140] Dr. Hill also inappropriately clusters standard errors by schools and across years. I explain why these standard errors are

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140. Dr. Hill inappropriately includes post-2015 Chicago data and excludes all 2023-2024 data. I describe why these changes are inappropriate in my October 7 rebuttal report. *See* Singer Rebuttal Report §III.A.2.c. I also discuss how Dr. Hill's latest critiques to my amended data are invalid in Part I.A, *supra*.

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inappropriate in Part I.B above. Nonetheless, Dr. Hill's methodology critiques are incorrect. I explain why each of these three critiques are inappropriate below.

B.   Dr. Hill's Criticisms of My EFC and Effective Institutional Price Variance Regression Are Invalid

80.   Dr. Hill's argues that my regression is based on inadequate data. He claims that because there an uneven distribution of the in-and-out-of-conduct data over the available years, the two situations cannot be compared.[141] As a result of the long-running nature of the Challenged Conduct and the limited data that Defendants provided for periods when they did not participate in the 568 Group, the vast majority of the data covers periods when both schools in a school pair participated in the 568 Group. Thus, any limitations result from Defendants' own data production. Nonetheless, such limitations do not undermine my analysis. In fact, only allowing years in which schools pairs occur both in and out of the conduct still yields negative, economically, and statistically significant EFC and Effective Institutional Price range associated with the Challenged Conduct.[142] Dr. Hill's critique is therefore invalid.

81.   Dr. Hill additionally claims that my methodology "provides no way to accurately disentangle the effect of the challenged conduct" from factors that vary year to year.[143] Dr. Hill provides no criticisms regarding the control variables I use, but rather seems to all but ignore their existence in leveling this inapposite criticism. In a footnote, Dr. Hill argues that my controls do not explain the majority of the variation in the range between expected family contributions.[144] He invokes this specter of missing explanatory variables to account for annual fluctuations without any suggestion as to what those differences might be. As explained in my Initial Report, my analysis

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141. Hill Surrebuttal Report ¶84.
142. *See* my workpapers for details.
143. Hill Surrebuttal Report ¶85.
144. *Id.* n. 91.

does not seek to maximize the "fit" of the regression line (i.e., explain the majority of the variation in the dependent variable.) I do not engage in any type of "curve fitting" exercise. Rather, my regression models aim to recover the causal effect of the conspiracy, and I chose independent variables to include in addition to the conspiracy flag with that goal in mind. Any variables that add to the explanatory power but do not confound the effect of the conspiracy would simply increase the "fit" of the regression. While such a result might generate palatable optics, it does not bear on my ability to recover the causal effect of the conspiracy. Even so, Dr. Hill identifies no other variables that I should have included but did not, nor does he actually include such a variable to demonstrate its effect.

82.   Dr. Hill asserts that the data are also invalid because some pairs of schools have higher variation between their EFCs and never appear during the conduct period, while other pairs have lower variation and appear mostly during the conduct period.[145] Dr. Hill does not explain why the Challenged Conduct would not influence these observed differences. In other words, he claims that the low variation in EFCs between school pairs that were in the 568 Group when compared with school pairs that were outside of the 568 Group illustrates that the data are invalid, rather than indicating that the Challenged Conduct influences this discrepancy. Dr. Hill provides no rationale for his expectation that something other than the Challenged Conduct results in lower EFC variation between Cornell and Duke compared to EFC variation between Johns Hopkins and Penn. Without any theoretical backing, this argument represents just speculation on Dr. Hill's part.

83.   Dr. Hill proposes adding school-pair fixed effects. He claims that systematic differences between school pairs motivate his decision.[146] Dr. Hill does not explain why, based on economic theory, he expects two schools to exhibit consistently different variation in EFCs when

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145. *Id.* ¶86.
146. *Id.* ¶89.

compared to another pair of schools. In other words, Dr. Hill suggests that students admitted to, say, Brown and Caltech would have an EFC (or Effective Institutional Price) variation that differs from that of students admitted to Brown and Cornell, without advancing any economic justification for this claim. Nonetheless, making this change to my methodology does not materially alter my results. I still find that the Challenged Conduct is associated with an economically and statistically significant lower variation in both EFC and Effective Institutional Prices.[147]

84.   Dr. Hill claims that the "expected impact of the conduct is not defined" for observations for which one Defendant admitted an applicant is a member of the 568 Group and the other Defendant is not a member of the 568 Group, meaning that we cannot tell if the conduct is expected to increase or decrease the range of EFCs for these mixed pairs.[148] He does not provide any support for this claim, nor does he expand on this claim. Instead, Dr. Hill proposes to simply remove all mixed pairs from the data.[149] Given that Dr. Hill's initial concern about my regression was the limited data outside of the conduct period, it is strange that his proposed solution to adjust for differences in data types is to remove approximately two-thirds of the data outside of the conduct period. Furthermore, his reasoning for removing the data is unclear. The question is not whether the mixed pairs differ from the pairs with both Defendants out of the 568 Group. The relevant question is whether any basis exists to claim that if one Defendant is in the 568 Group and another is out of the 568 Group, the Defendants would still be expected to coordinate on EFC and Effective Institutional Prices (and should therefore be considered part of the Challenged Conduct). Even if this were the case, it would make my results conservative. The results would be conservative because the mixed pairs would be attributing spillover effects from the conduct into the non-conduct period,

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147. *See* my workpapers for details.
148. Hill Surrebuttal Report ¶96.
149. *See* Dr. Hill's Surrebuttal workpapers.

which would be expected to decrease the variation in EFCs and Effective Institutional Prices in the non-conduct period. Nonetheless, making this change to my methodology makes fundamentally no difference to my results. I still find that the Challenged Conduct is associated with a statistically significant lower variation in both EFC and Effective Institutional Prices.[150]

85.   Dr. Hill makes methodological changes he deems necessary to my cross-admit EFC and Effective Institutional Price variation regressions. However, in order to show any notable change to my results, Dr. Hill's invalid dataset and standard errors are required. Therefore, Dr. Hill's arguments are moot. He effectively only claims that the exact value of the EFC and Effective Institutional Price variations are different from my original analysis, but that the Challenged Conduct is still associated with compressed and statistically significant variation. I also emphasize that this analysis does not stand alone in showing a causal effect from the Challenged Conduct. Rather, it buttresses my qualitative and quantitative findings regarding higher Effective Institutional Prices by indicating that the Challenged Conduct is also associated with reduced variation in the EFCs and Effective Institutional Prices between Defendants.

C.   Dr. Hill's Cross-Admits Critiques Do Not Undermine Economic Evidence Consistent with the Challenged Conduct

86.   By critiquing my analyses that variations in cross-admitted students' EFCs and Effective Institutional Prices, Dr. Hill is effectively asserting that the Defendants failed in fulfilling the express mission of the 568 Group: to narrow the "divergent" results in need analysis.[151] But if Defendants failed to achieve that result, which my statistical results show that they did achieve, Dr. Hill's results cannot provide a sound economic rationale, other than one consistent with collusion,

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150. *See* my workpapers for details.
151. YALE_LIT_0000013965 (describing "Value of 568 Membership").

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for why Defendants kept trying to do what they set out to do by continuing to attend meetings of the

568 Group, until November 2022, or shortly after the 568 antitrust exemption expired.

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Respectfully submitted on November 8, 2024,

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Hal J. Singer, Ph.D.

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APPENDIX 1: MATERIALS RELIED UPON

Bates Documents

Emory_568Lit_0006213

YALE_LIT_0000013965

Legal Documents

In Re Broiler Chicken Antitrust Litigation, Case No. 16-cv-8637, 2022 WL 1720468 (N.D. III. May 27, 2022)

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Caitlin Roman, *University Leaves Financial Aid Group*, Yale Daily News (Sept. 26, 2008), https://yaledailynews.com/blog/2008/09/26/university-leaves-financial-aid-group/

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Trial Materials

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Expert Report of Bridget Terry Long, Ph.D. (Aug. 7, 2024)

Expert Report of Lauren J. Stiroh, Ph.D. (Aug. 7, 2024)

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